immunity from any choleraic symptoms. The circumstance, however, that the microbe will not multiply in the intestines of some of the lower animals, and their immunity from cholera, together with the occurrence of the organism so largely in cholera-cases in man, may tend to support the view that it bears a relation to the disease in man, and that the lower animals owe their immunity from it to the inability of the microbe to develop in them.

In no case was there any fall in temperature whatever, nor, excepting a slight nausea in the one dog, and the death of the two guinea-pigs as recorded, was any appreciable effect apparent after recovery from the operations, which, as above stated, were performed by Mr. Horsley, antiseptically; and it is remarkable that a series of such serious operations as taking out a loop of the intestine and injecting it, should have been performed in these cases without any inconvenient results, and is a striking instance of the immense value of the antiseptic system of surgery. Everyone who has used rabbits for purposes of experiment knows how liable these animals are to septichæmia after operations—even the slightest—performed in the usual manner, but which in these cases were without any appreciable effect. This raises the strong presumption that, in those cases where, as stated by other observers, but without details of the method of operation, they have been followed by so-termed choleraic symptoms, this has really been due to septichæmia, for it would probably be quite impossible to perform any number of such operations upon rabbits, omitting antiseptic methods, without fatal results.

I had previously attempted to induce catarrh of the intestine in the animals to be experimented upon by drugs, but I found that, with rodents especially, their action was irregular, and would greatly complicate the results of the experiment.

With regard to the statement of MM. Nicati and Rietsch (*Comptes Rendus*, tome xcix, 1884, p. 928), that cultivations of the microbe emit the distinctive odour, somewhat etherial and not unpleasant, of cholera-evacuations, I have not been able to detect, in cultivations of any age, in whatever medium, any marked or distinctive fetor or odour, if, indeed, any such exists in the evacuations referred to.

With respect to this microbe being specifically distinct from all other forms, it is now amply shown that many identically similar, morphologically, occur in various situations, but it is asserted that their habits of growth are different and distinctive. This, however, is very doubtful. This character is as variable, under slightly different conditions, as are the form-phases of these organisms; the comma-bacillus, indeed, grows in the habit exactly described by Koch under certain conditions, that is, in nutrient gelatine at a given temperature; in other slightly altered conditions, its habit is widely different, and, if it be regarded, as shown in the case of Finkel and Prior's microbe, that the organisms are specifically distinct, it is not so with some similar forms that occur elsewhere; nor indeed, were this done, would it of itself suffice, by much, to prove that it does constitute the actual contagium of cholera; for, judged by the canons very clearly laid down by Koch himself (*Wundinfections-krankheiten, etc.*, p. 22), much beyond this is wanting to justify this conclusion. Koch's reports, indeed, raised a strong presumption in favour of its having a causal relation to the disease in which it had been observed, but the subsequent observations of the English Cholera Commission have greatly shaken this; and Dr. Klein's demonstration of cultivations of the comma-bacillus from the mouth, first noticed by Dr. T. Lewis, goes a long way towards disproving this, and has shown that the habits of growth of the two organisms are so very similar, that they can scarcely constitute a specific difference.

Its invariable occurrence in cholera cases is stated alike by both, but with respect to the numbers in which it is found, and that more especially in the most typical cases, the observations of the two are directly at variance, as is also the case with the most important point of their occurrence within the tissues of the intestine. Indeed, judged by the experience and explicit statement of Dr. Klein, the minute straight bacillus, which he has observed and described, has fully as good a title to be considered as constituting the virus as has the comma-bacillus. It cannot, indeed, be asserted that an organism, which is not proved to be pathogenic to the lower animals, may not, or does not, constitute the contagium in man; for, if this were admitted, we should be precluded from forming any final conclusion upon the etiology of such diseases as admittedly are not communicable to the lower animals. Nevertheless, in such cases, the difficulty of the investigation is increased, and more stringent proof that it fulfils the other requisite conditions is necessary.

That the comma-bacillus does not convey infection to man by inhalation, appears to follow from an experiment I have recently unconsciously made upon myself. In separating some other microbes by fractional cultivation in gelatine, I found that I had accidentally got a growth of typical comma-bacilli, as far as shown by the characters of the colonies, on the surface of the gelatine. This could only have arisen from atmospheric contamination, as the capsules and vessels employed were guiltless of previous cultivations, and the only instruments used were capillary pipettes, freshly drawn out. This occurred in the room of the laboratory in which I had been sitting several hours daily for some weeks past; and if present in the atmosphere in sufficient numbers to contaminate a cultivation contained in a small capsule, not exposed frequently, or for long together, it appears impossible that I, or others frequenting the room, could have avoided the introduction of some of these organisms into the respiratory passages, in whatever form they may have existed in the air.

To Dr. Koch belongs the eminent merit of having observed and described the comma-bacillus, which had hitherto escaped the notice of the numerous previous workers at this subject, and which, from their numbers and apparently invariable occurrence in this disease, deserves the most careful attention as one of its symptoms. His own investigations raised a strong *primâ facie* case that they did constitute the active contagium. This conclusion has not been confirmed by subsequent observation; neither, on the other hand, is its possibility finally disproved, and the nature of the virus must be regarded as still quite undetermined. It may perhaps be, as stated by Koch in his second official report from Egypt (*Deut. Med. Woch.*, 1883, No. 42, p. 616), that though the microbe bears some relation to the disease, it is not its actual cause. Any further opinion upon this point must rest, for the present, upon the final determination of the question whether the microbe is, or is not, specifically distinct from all other similar forms.

Beyond this question of the action of any particular organism or substance, lies the primary one, whether cholera is in any sense a communicable disease; this is yet by no means finally settled, and there are many circumstances patent to all residents in India, which must render this somewhat doubtful. Foremost amongst these is the state of the tanks of drinking-water, which has been referred to in connection with the occurrence in them of the microbe here in question; contamination of these with choleraic matter during the prevalence of the disease must invariably occur largely through the habits of the natives, more especially the Hindoos, both by bathing and constantly washing their linen in them; and were the disease essentially capable of propagation through the excreta, as frequently asserted and generally believed, the epidemic must spread much more rapidly and widely than often occurs. This consideration applies more strongly to the case of a figured or organic ferment, capable of reproduction and multiplication, than to a chemical or soluble one, which may be modified by large dilution; and this circumstance may tend to account for the discrepancy in the positive statements that have been made as to the occurrence of infection from choleraic matter in some cases, and the contrary.

This investigation was made at the Brown Institution, Wandsworth Road.

ON THE FIRST DISCOVERY OF THE COMMA-BACILLUS OF CHOLERA.

Read at a Meeting of the Royal Microscopical Society, March 11th, 1885.

By FRANCIS FOWKE, F.R.M.S.

LIKE many microscopists, being most deeply interested in the recent researches relating to the cholera-bacillus called the "comma-bacillus," by that eminent investigator Dr. R. Koch, of Berlin, it occurred to me to examine the current medical literature of the last epidemic of the disease in this country in 1849.

In the pages of the *Provincial Medical and Surgical Journal* and other periodicals for that year I found such evidence in the descriptions and corroborative woodcuts furnished by Drs. Brittan and Swayne, in their original and independent investigations, that I now venture to claim for them the priority in discovery of the cholera bacillus.

In laying before you some of the quotations and statements from various medical journals of that date in support of my views, I entirely disclaim all intention of impugning the originality and independence of Dr. Koch's recent discovery and researches upon this subject.

In the volume of the *Provincial Medical and Surgical Journal* for 1849, on page 600, will be found a report of a Subcommittee appointed by the Bristol Medico-Chirurgical Society to investigate the nature of cholera by means of microscopical observations. The Subcommittee consisted of Mr. James F. Bernard (Chairman), Mr. J. C. Swayne, Dr. Frederick Brittan, Dr. J. G. Swayne, Mr. Augustin Prichard, Dr. William Budd, Dr. J. A. Symonds, and Mr. John Cash Nield (Secre-

tary to the Microscopical Subcommittee). The report commences on July 9th, 1849. Dr. Brittan and Dr. J. G. Swayne having each examined specimens of rice-water evacuations which Dr. Budd had obtained from two patients in the Cholera Hospital, at the next meeting of the Subcommittee, they separately described and produced drawings of some peculiar bodies which they had noticed in those specimens. The descriptions and delineations given by these gentlemen coincided perfectly. The results of Dr. Brittan's separate observations are very remarkable. He examined a series of cases, from 3 to 20 inclusive, and published the results in his table in the *Medical Gazette*, for 1849, vol. xliv, pages 530 to 542. Dr. Brittan found some peculiar corpuscles to be constant in the intestinal discharges of cholera-patients; and similar bodies, but smaller, though well defined, were discovered by him in the matters vomited; they appeared larger and more compound in the dejections; decreased as the disease progressed favourably; and vanished with the disappearance of the symptoms. Dr. Brittan afterwards examined, under the microscope, specimens of healthy fæcal matter, and the fluid stools of typhus, typhoid, and other diseases; but failed to detect anything corresponding with the peculiar corpuscles belonging to cholera-dejections, though he discovered these bodies in cases of severe choleraic diarrhœa. From these observations, he inferred that the bodies in question were peculiar to cholera, and bore some essential relation to the disease.

Dr. Budd, in a pamphlet on "Malignant Cholera," reported that he detected bodies. identical with choleraic corpuscles, in drinking-water obtained from cholera-districts. He says: "Shortly afterwards, and being at the time aware of this discovery, I detected the same organisms, in great numbers, in almost every specimen of drinking-water which I was enabled to obtain from cholera-districts. First, in the drinking-water from Wellington Court, Redcross Street, where cholera first broke out (with any violence) in Bristol; subsequently, in the water of the Float, and in the drinking-water from King Street, in the same city; since then, again, in London, in water from Lovegrove Street, and from the Surrey Canal; and, lastly, in drinking-water from the Workhouse at Stapleton, commonly known by the name of the French Prison; being all places where, at the time the water was obtained, cholera was making dreadful havoc. This led me to examine a great number of specimens of water from healthy quarters; and, although I often found in it a good deal of matter of various kinds, organic and other, in no single instance did I see anything resembling the peculiar bodies in question."

These cells, annular bodies, or corpuscles, are described as follows. "They vary very much in size and apparent structure during the different stages of their development. The smallest are of the same size as, or even much less than, blood-globules, so that to show them properly an object-glass of high magnifying power, such as one-eighth, one-twelfth, or one-sixteenth of an inch is required; their walls refract light powerfully; fragments of them present the appearance of *small segments of circles*." The italics are mine.

Such an important discovery as these peculiar corpuscles did not go unchallenged, for the then President of the Microscopical Society, Mr. Busk, endeavoured to show that the large bodies were nothing more than a species of uredo, a kind of smut frequently present on wheat, and that the smaller annular bodies were not sporules or an earlier stage of development of the large bodies, but starchy granules derived from the bread eaten by the patients. He concluded his last report, "we are not able to account for the origin of those peculiar discs;" but whether Mr. Busk means by this the large ones or the small ones, it does not state. Dr. Plomley (page 615 same *Journal*) in a criticism on the discovery, makes out, after examination of the cells, or fungoid bodies, that they are nothing more than altered epithelium-cells mixed with an unusual quantity of their nuclei and granules. During this discussion on Cholera, its treatment, etc., the editor of the *Provincial Medical and Surgical Journal* had to more than once interfere, and to write strong leaders on the pitiful spirit of ungenerous rivalry the critics exhibited (*vide* page 630). It appears that the Royal College of Physicians appointed Drs. Baly and Gull to examine into the truth of the new theory. The editor of the *Journal*, in a kindly article upon the subject, after mentioning that so eminent a microscopist as Mr. Busk distinctly denied the *peculiarity* of the cells, but maintained that one variety was no more or less than the common uredo or smut of wheat, and the other might be taken for modified blood-discs,[1] also adduced the opinion of the Microscopical Committee of the Royal College of Physicians on the evidence, as tending in more direct terms to exhibit the fallacy of the new theory, and then ended his leader as follows. "One cannot help feeling some regret that observations so honestly made and candidly stated, should be apparently doomed to such speedy discomfiture; but the lesson may at least be learned, that the science of medicine is, less than any other, likely to be advanced by hasty generalisations; that it is, on the contrary, a science which, *par excellence*, requires the exercise of the closest induction, while flights of fancy, even the most brilliant, will ever fail to inscribe in its annals the valuable and the true."

On page 657 of the same journal is a leading article calling the attention of the readers to the report of the Bristol Medico-Chirurgical Society, and that number of the *Journal* contains a reply by Dr. Swayne to the report of Drs. Baly and Gull,[2] on the choleraic bodies discovered by Dr. Brittan and himself. Dr. Swayne notices the total omission from the report of any reference to the number of cases from which these observations were deduced, and says that Dr. Brittan and himself had obtained their results from more than sixty cases. In this leader, the editor states that Dr. Swayne has, he thinks, "succeeded in making good his defence, and it remains for future experimentalists to disprove or confirm the presence—whether constant or occasional—of these bodies. Meanwhile, instead of carping and cavilling at the accuracy of these asserted discoveries, it is incumbent rather upon the profession to show their gratitude to the Bristol Medico-Chirurgical Society, and more especially to its Microscopical Committee, for the industry, zeal, and talent displayed in the prosecution of this interesting subject. It is true that an over active imagination may sometimes require the curb; but if all inquiry is to be thus repressed, and observations, made with great toil and some danger, are to be at once denounced as unworthy of credit, much injury to science will inevitably result, and labours which can only be made endurable by the approbation of mankind will, in an arduous profession like ours, be avoided by all but those who make them the business of their lives."

In the report of the meeting of the Bristol Medico-Chirurgical Society, the President, in taking the chair, made some remarks upon the discovery of Drs. Brittan and Swayne; he considered that the evidence remained in favour of the specific connection of these corpuscles with cholera, as shown by the careful examinations and experiments of these gentlemen, and that the support given by the opinions of such eminent microscopists as Dr. Carpenter and Mr. Quekett, was of greater value than the negative results of Drs. Baly and Gull and Mr. Busk.[2]

Dr. Swayne also stated that the bodies represented in the antagonistic report as present in the dejections of other diseases were too indefinite in form to be confounded with the cholera-bodies. Dr. Swayne illustrated his reply by exhibiting, under several microscopes, numerous specimens of perfect and imperfect cholera-cells, together with the uredo from bread and wheat, fatty bodies from cheese, calcareous particles from chalk, etc.

In a leading article in the *Lancet* for October 13th, 1849, page 406, are the following remarks.

"The profession has now before it the chief data respecting the discovery of the fungoid bodies which are believed to be peculiar to the cholera-evacuation. Drs. Brittan and Swayne, in the first instance, simply observed the cells which they have described as peculiar to the evacautions in cholera. Dr. Brittan next, on the suggestion, we believe, of Dr. Bernard, condensed the air of rooms in which cholera-patients had died, and detected similar or identical bodies in the cholera-atmosphere. Dr. Budd also detected similar cells in the waters of infected localities. Dr. Thomas Williams has also been engaged in the microscopical examination of cholera-stools, and has published confirmatory results. Thus far, everything tends to the confirmation of the primary discovery of Drs. Brittan and Swayne; and again we may, however, state as our opinion that the first fact is the really important fact, and for the discovery of that, there can be no doubt we are indebted to the joint investigation of Drs. Brittan and Swayne.

[1] Mr. Busk, in a letter to the *Medical Gazette*, vol. xliv, p. 760, withdrew his statement that the smaller annular bodies were altered blood-discs. His letter is quoted, as follows. "Since that letter was sent, I have been kindly informed by Dr. J. W. Griffith (Editor of the *Micrographic Dictionary*) as to certain chemical characters of the small annular bodies, which would seem to make it impossible that they should be altered blood-discs, as I supposed simply from their form. As I have had no opportunity of applying chemical reagents to them, my erroneous supposition is the more excusable. The statement of their true nature, I of course leave to those to whom the discovery is due."

[2] In the report of Drs. Baly and Gull, the following statement will be found on page 353 of the *Medical Times*, 1849, vol. xx. "Intermediate between these and the third class of bodies are minute oval or round corpuscles, which have an annular appearance, but on close inspection are seen to have their area filled up with a transparent substance, presenting sometimes perforations. In some specimens of rice-water fluid, oval bodies, in part having their middle filled up as here described, and in part mere rings, exist in extraordinary abundance. The rings of these bodies have been observed by Mr. Busk and Dr. Griffiths to be divided by cross lines into segments."

The first perception of the peculiar cells was the great fact. The subsequent discovery of cells in the atmosphere and water, however interesting, is altogether secondary, and conveys no merit similar to that which connects itself with the first discovery. We make these remarks in a perfect spirit of fairness, because we see in some papers Dr. Budd's name is mentioned alone, and in others, as in the series of articles in the *Morning Chronicle*, the name of Dr. Swayne is *studiously* suppressed."

On page 493 of the *Lancet* are the principal conclusions of Drs. Baly and Gull, the Cholera Subcommittee of the College of Physicians on the cholera-fungi.

"1. Bodies presenting the forms of the so-called cholera-fungi, are not to be detected in the air, and, as far as our experiments have gone, not in the drinking water of infected places.

"2. It is established that, under the term annular bodies and cholera-cells or fungi, there have been confounded many objects of various and totally distinct natures.

"3. A large number of these have been traced to substances taken as food or medicine.

"4. The origin of others is still doubtful, and these are clearly not fungi.

"5. All the more remarkable forms are to be detected in the intestinal evacuations of persons labouring under diseases totally different in their nature from cholera.

"6. Lastly, we draw from these premises the general conclusion that the bodies found and described by Messrs. Brittan and Swayne are not the cause of cholera, and have no exclusive connection with the disease; or, in other words, that the whole theory of the disease, which has recently been propounded is erroneous as far as it is based on the existence of the bodies in question.

(Signed) "William Baly, M.D. } *Cholera*
"William W. Gull, M.D. } *Subcommittee.*"

In a review, in the second volume of the *Medical Times* for 1849, of a pamphlet on Malignant Cholera, by Dr. W. Budd, the author is quoted as stating that he was led to regard the organism as having a relation to malignant cholera by the following considerations.

1. By the characteristics of the thing itself, which, by showing it to be possessed of a complex organisation having a definite mode of development, a specific form, and spontaneous power of growth and multiplication, stamp it at once as being of distinct species.

2. By its constant presence in cases of cholera, and its absence (with a few casual exceptions, in which a small number of stray bodies of similar character are found) in other diseases.

3. By its presence in the discharges in such infinite numbers.

This last consideration, when taken with the two former, is very striking; for, assuming the thing itself to be a definite living organism, and therefore of extrinsic origin, it becomes impossible to conceive that its presence in such countless numbers in the rice-water can be a matter of chance, a mere incident, an epiphenomenon, or anything, in fact, short of an essential character of the disease; and if essential, many other considerations are at hand to declare that the relation thus inferred to exist can only be one of cause and effect. The review of Dr. Budd's pamphlet, which appears to be an exceedingly able one, sums up the argument as follows.

It is assumed—

1. That the peculiar bodies are fungi.

2. That they exist constantly in cholera-discharges, and not elsewhere.

3. That, by reason of their complex organisation and of their number (taken with their invariable presence), they must be the cause of cholera.

4. That, as the fungi are introduced in small numbers, and are voided in myriads, they must be increased within the body, at the expense of that body, and live; the drain of fluid thus resulting producing all the other symptoms.

5. That the constant presence of the fungi in the air and water of infected districts affords evidence confirmatory of these inferences.

On page 351 of the same volume of the *Medical Times* will be found in full the report of the Cholera Committee of the Royal College of Physicians of London by their Subcommittee, Drs. Baly and Gull, on October 17th, 1849. I merely mention this for the information of anyone who wishes to read it; it is much too long to reproduce, and the principal results arrived at by the Subcommittee have been quoted.

The editor of the *Medical Times* takes the view of the College of Physicians, congratulates them on the zeal and ability which their committees have shown in this matter, and considers the question closed.

Dr. Koch, it is stated on page 596 of the Royal Microscopical Society's *Journal* for 1884, has presented to the German Government six reports on the cause of the cholera-epidemic, as the result of investigations on the excreta, and on the dead bodies themselves, of cholera-patients in Egypt and in India. The internal organs, lungs, liver, spleen, kidneys, etc., as well as the ejecta, were found to swarm with microbia of a great variety of kinds; in all cases was found one definite kind of bacillus. This was found in largest quantities in the tubular glands of the intestines, especially between the epithelium and the membrane of the gland. This particular form was also never found in the intestines or in the ejecta of those not suffering from cholera.

The cholera-bacillus is not quite straight, but is somewhat curved, in the manner of a comma, or even nearly semicircular. In cultivation, there often arise S-shaped figures, and shorter or longer slightly wavy lines.

As to the question whether their presence is simply due to the presence of the choleraic disease, which promotes their growth and development, or whether they are themselves the cause of cholera, Dr. Koch is very strongly of opinion that the latter is the true explanation, since they are never found either in the organs or the ejecta except in the case of patients who have died of, or are suffering from, cholera. They are also found in that organ which is the seat of the disease, namely, the intestines; in the first feculent ejecta, the bacilli occur only in small quantities, while in the liquid odourless ejecta they occur in enormous quantities, all other kinds of bacteria being almost entirely absent; they diminish in number as the excreta become more feculent, and have entirely disappeared when the patient is completely restored to health.

The original illustrations to the report of the Microscopical Committee of the Bristol Medico-Chirurgical Society I have had reproduced. They have been photographed direct on to the wood-block, from which the illustrations were then cut. They are figures 2 and 4 in the original, the first representing the cholera-bodies from the air and water of infected places, the second representing the cholera-bodies from vomited matter.

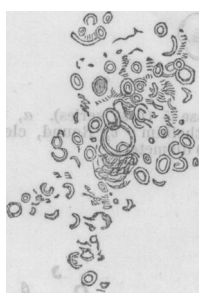


Fig. 2.

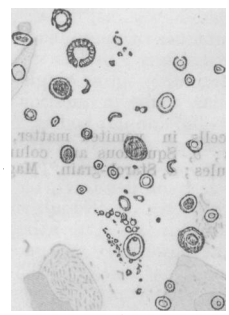


Fig. 4.

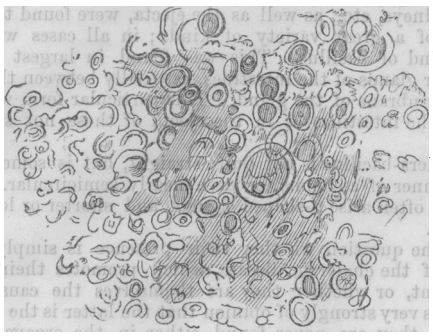
These can be compared with the accompanying photomicrographs of the acknowledged comma-bacillus of Koch, also with a magnified camera-lucida drawing from a photograph, and with a diagram and sketch from memory, by Dr. Maddox, of the peculiar kind of growth described by Dr. Klein before the Royal Society, and substantiated by his beautiful preparations and cultures, in order that the Fellows of this Society may judge whether they resemble the original figures or not.

My reasons for bringing forward this page of forgotten history in the study of the disease of cholera, is not only the interest attached to the way in which the question of the fungoid character of the disease was medically and publicly discussed in 1849, but principally to show, as far as can be now ascertained from the above report, that the comma-bacillus was known and recognised so far back as thirty-five years since, the discovery being made by two Englishmen, Drs. Brittan and Swayne. I would ask you also to compare the description of these bodies by Dr. Brittan in the first paragraph of my history and that of Dr. Koch in the latter part.

I am aware objections may be taken as to the exact shape, size etc., of the organisms figured, and the ordinary appearance of the comma-bacilli of Koch; but it must be remembered these greatly differ according to the mode of culture, age, etc. Still, I believe no unprejudiced person would hesitate to say that the similarity, considering all

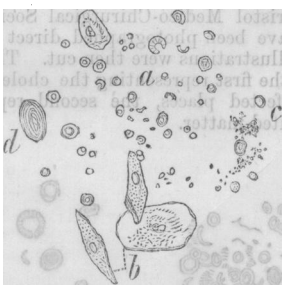
the conditions of less perfect instruments and methods of observation then in use, was not accidental, but real.

The subjoined woodcut is copied from illustrations to Dr. Brittan's paper in the *Medical Gazette*, page 531, vol. xliv.

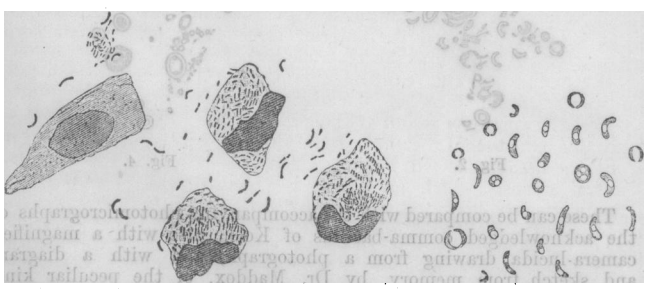


On page 531, vol. xliv, of the *Medical Gazette*, is a note to the article of Dr. Brittan: "On every opportunity that I have had of examining the intestines of those who died from cholera, these bodies have been found adhering to the mucous membrane in shreds of white matter, and very abundant; and the inference is, that in these very rapid cases they are in the intestines, though not given off in the evacuations."

The following is copied from illustrations to Dr. Swayne's paper in the *Lancet*.



Cholera-cells in vomited matter, from Case 5 (first series). *a*, Cholera-cells; *b*, Squamous and columnar epithelium; *c*, Round, clear, oily globules; *d*, Starch-grain. Magnified 420 diameters.



Dr. Klein has most kindly lent two of the woodcuts which illustrate the chapters on the cholera-bacillus, in the new edition of his most interesting work on *Micro-organisms*, which will be published next week by Messrs. Macmillan & Co. These, which are given here, can be compared with the illustrations of Drs. Brittan and Swayne, of 1849.

THERAPEUTIC MEMORANDA.

CARBOLIC ACID IN INDIGESTION.

I HAVE lately treated several cases of indigestion with carbolic acid, and the results have in each instance been so fortunate, that I am anxious to add the results of my experience to those of Mr. Dixon. I have found it most useful in that form of dyspepsia known as fermentative, accompanied by constant sour rising and eructations of gas, with pain after meals, and discomfort even after drinking milk or cocoa. My attention was first directed to it by Dr. Fenwick, who gave the glycerine of carbolic acid (1 part of crystallised carbolic acid to 4 parts of glycerine). The dose is from five to ten minims in mint-water or other convenient vehicle. As it mixes well, I think it a more safe and elegant form than a solution of the acid in water only. When there is much pain of the stomach after food, I have found it useful to add five or six minims of the liquor opii sedativus to each dose; and, when there is want of tone in the seat of digestion, and bad appetite, five to ten minims of the tincture of nux vomica will often be found serviceable. I have found these remedies also very valuable in the above combination in cases of pyrosis, where, I think, the sedative influence of the carbolic acid on the mucous membrane is far more useful than the bismuth one usually given in such cases. It is an interesting subject of inquiry whether the carbolic acid acts by arresting fermentative changes in the stomach, or by its well known anæsthetic influence on mucous membranes. I have long given one-grain pills of this remedy in cases of vomiting from various causes, and have rarely found it fail to arrest it. In some of these cases there was no fermentative condition of the contents of the stomach; some of them were cases of reflex vomiting; yet all were, with few exceptions, greatly benefited. It would be desirable that the subject should be still further discussed by those who have had experience of the drug. EDWARD BERDOE, M.R.C.S., L.R.C.P.Ed., etc.

OBSTETRIC MEMORANDA.

TREATMENT OF NEGLECTED SHOULDER-PRESENTATION.

ON January 7th, 1885, I was called to attend Mrs. L., aged 23, in her second confinement; it being late at night, and three miles away from home, I took my bag of instruments with me.

On arriving at the house, I was told by an old woman, whom I afterwards ascertained to be the midwife, that the patient had been in labour twenty-four hours, "but that she was not bad enough yet, as she had no pains worth mentioning."

On making an examination, I found the hand and arm in the vagina, and the uterus firmly contracted on the body of the child. I afterwards was told that the midwife had said in the morning that "all was not right, that there was a part in the passage, but that it would come all right yet."

I then anointed my left hand and arm with a mixture of vaseline and carbolic acid, passed it into the vagina, and attempted to return the arm; but, as usual, I found I could not do more than bend the arm up, but failed to alter the presentation of the body of the child. I then gradually, but with some difficulty, passed my hand into the uterus, which I found closely applied to the body of the child, the whole of the liquor amnii having escaped. I ultimately found a leg, and brought down the foot, on which I made considerable traction; at the same time, I got one of the women to make gentle pressure on the abdomen, to assist version; but I found that the more traction I used the more firmly the fœtus became wedged in the pelvis, and the arm and shoulder would not recede. I then asked for a skein of worsted, but, as this could not be got, they found me some very broad linen tape. I made a hitch of this, passed it over the foot, and tightened it above the ankle. I then took the tapes in my right hand, and passed my left into the vagina up to the uterus; then, as I used traction upon the leg by means of the hitch, I gradually pushed the shoulder into the uterus, then the arm, and by this means the turning became quite easy; the rest of the labour was conducted on general principles. The child, a well developed female, was dead. The mother made an excellent recovery.

The principal points of interest in this case are these. First, attempts to replace the arm in cases where the liquor amnii has escaped are useless and cause much pain; in no case have I been able to replace it, and thus alter the presentation. Secondly, by attaching the tapes to the leg, and using traction on them, it left room for my other hand in the vagina, by which means I was able to act directly on the two poles of the body at the same time. I feel sure, by this method, the dangerous operation of dismembering the child, as recommended by Dr. Donaldson, may be avoided; in many cases, the life of the child might be saved, and that without any risk to the mother.

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LARGE BEQUEST.—Mrs. Mary Fletcher, of Burlington, Vermont, has left 200,000 dollars (about £50,000) to a hospital which she founded.

THE suicide of a medical student is reported to have occurred at St. Louis, Missouri. Failure to pass his examination is assigned as the cause.